most closely related to *Cenchrus*. With respect to *Olyra,* the evidence is overwhelming that this genus is bambusoid. In 1947, Virginia Page (Bull. Torrey Bot. Club 74:232–239) demonstrated convincingly that the leaf anatomy of this genus is of the bamboo type. My investigations of the embryo (Amer. Jour. Bot. 44: 756–768. 1957) indicate that the affinities of this genus are bambusoid. The embryo in the Paniceae is very different. Most students of the Gramineae agree that the embryo characters give perhaps the most powerful evidence regarding affinities of genera in this group. Another indication that *Olyra* is bambusoid comes from cytology. Chromosome counts of *O. latifolia* by Reeder et al. (Taxon 18:441,442. 1969) and by Pohl & Davidse (Brittonia 23:293–324. 1971) gave $2n = 22$; *O. yucatana* was found to have this same number by Tateoka (Bull. Torrey Bot. Club 89:77–82. 1962); and $2n = 22$ was also reported in *O. loretensis* by Gould & Soderstrom (Canadian Jour. Bot. 48:1633–1639. 1970). This chromosome information supports the contention that *Olyra* has bambusoid affinities. Basic numbers of $x = 12$ or 11 are usual among bamboos and their kin, whereas $x = 9$ or 10 is characteristic of the Paniceae.

Another character of considerable importance in grass taxonomy is the hilum. In all members of the Paniceae, this structure is punctate; in Bambuseae it is linear. Examination of "seeds" of *Olyra* shows a hilum which is distinctly linear, clear evidence that this genus was wrongly placed in the Paniceae by Hitchcock & Chase. According to R. W. Pohl (pers. comm.) another bambusoid grass, *Streptochaeta,* has a distinct "germination flap" on the fertile lemma.

To suggest that *Olyra* belongs in the Paniceae merely because it has a "germination lid," and to ignore the other overwhelming contrary evidence is certainly naïve. This sort of reasoning led agrostrologists of the past to group *Eragrostis* with *Poa*. A more satisfactory explanation for the occurrence of a "germination lid" in panicoid genera and in *Olyra* would seem to be not that this indicates close phyletic relationship, but rather that it is a case of parallelism. If the caryopsis is tightly enclosed within an indurated lemma and palea, there must be a means for the rootlet of the embryo to emerge at germination if the seedling is to become established. One would expect natural selection to favor those plants which developed a weak spot ("germination flap") directly above the position of the rootlet. The enhanced germination would permit an increased number of offspring.—JOHN R. REEDER, Department of Botany, University of Arizona, Tucson 85721.

REVIEW

Marine Algae of California. By ISABELLA A. ABBOTT and GEORGE J. HOLLENBERG. 827, 701 figs. Stanford University Press, Stanford, California 94305. 1976. $22.50.

The publication of this massive contribution has been awaited eagerly by marine phycologists in California and elsewhere. The authors are noted for their major contributions to the systematics and morphology of Pacific marine algae. In some respects the book's format is similar to that of Smith's Marine Algae of the Monterey Peninsula published in 1944 which it now supplants. MAC, however, contains a number of very important features not present in Smith.

The extensive introduction has information on classification, form and physiology of marine algae, a brief geographical treatment of the California coast with simplified maps indicating major coastal landmarks and suggestions about techniques for collecting and preparing specimens. Following the introduction is one of the book's highlights—a 25 page account by G. F. Papenfuss entitled Landmarks in Pacific North American Marine Phycology, which emphasizes the early expeditions and botanists who provided our first knowledge of the remarkable flora on this coast. It includes photographs of several of the more noted phycologists associated with the flora, memorable anecdotal information about many, as well as mention of algal taxa named in their honor.

The main text of the book is divided into four sections treating the genus *Vaucheria* (Chrysophyta), and the phyla Chlorophyta, Phaeophyta and Rhodophyta. Keys and descriptions for the orders, families, genera and species are included. Illustrations of each species are on the page adjacent to its description. The main text is followed by a master dichotomous key to the genera of the three major phyla, a glossary, ilterature cited, and a taxonomic index.

In the preface the authors explain their rationale for the book's format and content. It is intended to serve as a manual for identification by students with minimal training in lower plant biology. Conservation of space was essential to keep the number of pages and price to a minimum. Consequently, descriptions are "concise", generally having somewhat less information than those in Smith. The authors chose 1972 as the cutoff date for changes and additions in the manuscript. As a result of this, new taxa, additions to the flora and nomenclature changes published since 1972 are not included.

Perfection is an elusive quality, seldom achieved, and although the book is a remarkable achievement it is not perfect. I would like to point out a few of its imperfections and positive features.

In the introduction (p. 7) several questionable statements are made. First, the authors state:

> "The geographic area whose algal population is most like that of California is Japan. The marine flora of Northern Honshu and Hokkaido would seem very similar to a Californian . . . It is estimated that about 30–40 percent of the species occurring in California may also be found in Japan. This is a larger percentage than a comparison of California with the North Atlantic would realize, though temperature barriers in the Pacific are no less formidable as obstacles than the land barriers separating California from the Atlantic."

The figure of 30–40 percent species shared in common is a gross overestimate. Hommersand (1972, Proc. Intern. Seaweed Symp. 7:66–71) very generously estimates that perhaps 110 species are common to both California and Japan. This is only 16% of the 669 species listed in MAC. A more realistic figure based on hardcore evidence might reveal less than 10% are common between the two areas. Their statement that temperature barriers are no less formidable obstacles than land barriers cannot be taken seriously. Many temperate and cold water marine algae can withstand wide ranges in temperature (5–20°C) for several days or weeks and still remain viable. This seems a sufficient time for drift algae to be transported short distances and allows for progressive establishment along a coast line. Moreover, they apparently do not consider the possibility that migration of certain species may have occurred at times when water temperature patterns differed from those of the present, nor do they consider the possible involvement of continental drift in species distribution.

In the last paragraph on page 7 the authors state:

> "Although California and Japan share many genera, there are usually larger numbers of species in given genera on one side of the Pacific than on the other, implying genetic if not ecological diversity. One genus, *Laurencia,* has 12 eastern Pacific species and 23 western Pacific species."

This is an unreasonable comparison because *Laurencia* is principally of tropical and subtropical distribution. In these particular ocean temperature zones there is a vastly greater coast line in the western Pacific than in the eastern Pacific. The greater number of habitats in the western Pacific should permit a greater species diversity. By contrast the coast line in temperate and colder water zones is closer to being equal. Surprisingly, the authors do not consider upwelling as one of several possible factors influencing species diversity, yet that is one of the factors considered by zoogeographers in explaining the greater diversity of the Pacific North American biota relative to the Atlantic North American biota.

The integration of illustrations with species descriptions, the generally high quality of reproduction of the figures, the illustration of every species and most subspecific taxa included as well as the use of figures to show important diagnostic characters are all very useful features which make this flora vastly superior to others I have used. However, there are also a number of problems in regard to the figures. The single most cumbersome and time-consuming aspect is that the figure numbers are not directly next to the figure (although each figure in a composite plate is identified in the caption by a number and its position on the page, e.g., lower right, upper center). Moreover, figure numbers are not cited with the species descriptions. For rapid accurate reference it is essential that figure numbers be placed immediately next to the figure and, what is especially important, that they be cited in the description for each species. The captions generally fail to identify the types of reproductive structures illustrated. For the inexperienced student, to whom this flora is especially directed, these deficiencies could be very troublesome and frustrating.

In Fig. 23 (*Lithothamnion californicum*) the caption identifies the figure as a section through a tetrasporangial conceptacle, yet the figure shows binucleate bisporangia. Tetrasporangia but not bisporangia have been reported for this species. For Fig. 156 (*Punctaria hesperia*) the caption indicates that only the "unangia" are illustrated but "plurangia" are also present in the figure. Fig. 310 and Fig 311 were transposed (Abbott, personal communication). Fig 310 is of *Peysonnelia rubra* var. *orientalis* instead of *P. profunda* as indicated in the captain. Surprisingly, the description of *P. rubra* var. *orientalis* does not refer to the zonately divided sporangia of this taxon, a unique character among the California species of *Peyssonnelia*. This would have been useful as a keying character and should have been mentioned in the generic description as an exception to the cruciately divided sporangia.

I should note here that misspellings are very infrequent but one is Peyssonelliaceae and *Peyssonellia* (sic). The accepted spelling is Peyssonneliaceae and *Peyssonnelia*.

Some descriptions tend to be incomplete or inaccurate with respect to information on reproduction. For example, that of *Farlowia* includes a description of tetrasporangia, the implication being that tetrasporophytes and gametophytes are ismorphic. This report of tetrasporangia is based on Abbott's description of tetrasporangia in *Leptocladia conferta* which she transferred to the genus *Farlowia* in 1968 (J. Phycol. 4:180–98). However, tetrasporophytes are not recorded for any other species of *Farlowia*. It may be possible to return *F. conferta to Leptocladia* on this basis, although the female reproductive structures of *F. conferta* are more similar to those of *Farlowia* than of *Leptocladia*. It appears that California species of the crustose genus *Cruoriopsis* may be the tetrasporophyte for at least two species, *F. compressa* and *F. mollis* (DeCew and West, unpublished observations). In contrast, the description for *Gymnogongrus* states correctly that a tetrasporangial phase is unknown in certain species.

The red algal genus *Besa* was placed in the Phyllophoraceae, although it is traditionally placed in the Gigartinaceae. The authors do not explain the basis for this transfer.

The northern limit of the geographic range for *Pachydictyon coriaceum* is given as Cape Arago. This is, however, based on confusion in identification and synonymy discussed by Dawson (1950, Wassman J. Biol. 8:267). The taxon involved is *Dictyota bringhamiae*. Furthermore, the description of *P. coriaceum* in MAC indicates that the thalli are 400–500 μm thick, yet the scale shows a maximum thickness of 300 μm in the specimen illustrated.

The authors state on page 119 that:

> "Scagel (1960) and Chihara (1960) indicated the "*Collinsiella*" may be a stage in the life history of a species of *Enteromorpha* and/or Monostroma."

It is correct that Scagel considered *Collinsiella* as a stage in the life history of *Enteromorpha*, but Chihara's work in no way suggests this. The three species investigated by Chihara have multicellular gametophytic stages which are cushion-shaped or open sacs whereas the sporophytic stage is a unicellular "zygocyst" resembling *Gomontia*. Nowhere does Chihara state that *Collinsiella* is a stage of *Monostroma,* although it is known that *Gomontia*-like stages occur in the life histories of some species of *Monostroma*.

In general the keys seem quite workable and are less troublesome than many of those in Smith. However, it should be noted that the second part of the third dichotomy of the green algae key (p. 748) is incorrect and confusing. The genera of the Prasinophyceae, Chaetophoraceae, Ulvaceae, Ulotrichaceae and Monostromataceae to which this choice leads are certainly not coenocytes.

The use of quotation marks with "Chlorochytrium" (dichotomy 30 of the master key of green algae) implies that the authors do not accept the genus as being taxonomically valid, probably because *C. inclusum* was shown to be the sporophytic stage of a species of *Spongomorpha* by Chihara (1969, Phycologia 8:127–33) and other workers. However, this does not invalidate the genus because the life histories of the type species, *C. lemnae* (an endophyte of *Lemna* in freshwater), and other species, including *C. porphyrae* have not been investigated yet.

The first dichotomy of the *Acrochaetium* key (p. 309) does not enable one to identify *A. rhizoideum* because the erect filaments are considerably longer than the endophytic system. In addition, the illustration of *A. rhizoideum* (Fig. 260) does not show the diagnostic feature of the species, namely the numerous pyrenoids in each cell. Other California species lack a pyrenoid or have a single pyrenoid per cell.

Although there are minor deficiencies such as those mentioned above, the Marine Algae of California is exemplary in its overall quality and will serve phycologists well for many years. The authors and publisher should feel pleased with their efforts.—JOHN A. WEST, Department of Botany, University of California, Berkeley 94720.

REVIEW

The Tactless Philosopher. Johann Reinhold Forster (1729–1798). By MICHAEL E. HOARE. x + 419, 13 illus. Hawthorne Press, Melbourne, Australia, 1976. $15.95. Austr.

The author citation "Forst." (or more accurately Forst. & Forst. f.) is unfamiliar to most California botanists. However, *Dichondra* is one of their 75 new genera published in *Characteres genera plantarum* (1776), which included the first description of New Zealand plants, Queen Charlotte Sound being the probable source of *Dichondra repens*. The want of a "full-length biography" was noted by Michael Hoare in his sketches of the Forsters, father and son, in *Dictionary of Scientific Biography* (1972). Now this thoughtful, accurate, and attractive account of the foibles and fortunes of a fumbling Forster of two centuries ago is highly recommended.

E. D. Merrill was Forster's "most trenchant critic" of botanical matters. His condemnation of Forster's use of Solander's generic names is, in Hoare's opinion, unjustified and could not have rested on access to the Banks and Solander specimens